admit that we have thus far given only short and incomplete accounts of the many kinds and cases of intersexuality encountered in our material. We acknowledge and regret, and are steadily supplementing, this incompleteness. But morphology, beloved of Goldschmidt, is I presume adequately represented by oviducts in males (*Anat. Rec.*, 1925, 31, p. 349); by persistent, even functional, right ovaries in females (*Amer. Nat.*, 1916, 50); and by the hermaphrodites listed (Whitman, 2, 1919), or referred to in connection with rather full descriptions of some other abnormal (possibly not intersexual) gonad conditions (*Brit. Jour. Exp. Biol.*, 1925, 2). If these, as yet little described, cases of hermaphroditism should lead our critic to dispose of them by the further assertion that Riddle can not properly recognize an hermaphrodite he is entirely welcome to that position.

Goldschmidt states that our "claim to the experimental production of sex-reversal by reproductive overwork and by crossing . . . is based on the assumption that the first egg of a clutch is male, the second female." This is simply not true. "Our studies on 'sex control' manage to get on whether the eggs come in normal order, reversed order or utter disorder" (*Amer. Nat.*, 1925, 59). Also, according to Goldschmidt we have "never proved experimental sex-reversal or made it even probable." Waiving the large question of proofs, we may note that calculation of probabilities in a single result obtained in one of our very few "family" crosses indicates that—apart from sex-reversal—this result "could be expected to occur only once in 9,384 trials" (*Anat. Rec.*, 1925, 31). So apparently, either Goldschmidt must read more, or in my items of data I must eliminate part of *one* chance in 9,384.

To say that "Riddle's theory of sex determination by different metabolic rates . . . fails in the normal case of male heterogamety; it fails in such cases of female heterogamety as the gipsy moth, etc.," is merely to use words without meaning. The theory was founded upon forms showing "female heterogamety" (pigeons), and early applied, successfully we think, to forms (frogs) which later proved to show "male heterogamety"; moreover, as earlier pointed out, parts of this metabolic theory were later borrowed and lugged unacknowledged into Goldschmidt's own theory of sex-determination in the gipsy moth.

Well or ill founded—and much in addition to work with pigeons forms part of its foundation—there exists a vigorous quantitative theory of sex, based on real or fanciful sex-reversal and intersexuality apart from zygotic composition (on which Goldschmidt's studies are based), and on measurements of metabolic sex distinction in all stages—ovum to adult. We and others have taken a good or a bad part in all this, and the quantitative theory of sex can not be properly discussed—as Goldschmidt would have it—as the private affair of the "Columbia school" and the laboratory of Goldschmidt.

Oscar Riddle
Carnegie Station for Experimental Evolution,
Cold Spring Harbor, L. I.

ZOOLOGICAL NOMENCLATURE

Referring to the recent referendum on Dr. Poche's (Vienna, Austria) three propositions in regard to the Rules of Zoological Nomenclature, the undersigned has the honor to report to the zoological profession the following results of the ballot:

Poche's proposition I:	8 votes for; 549 votes against.
Poche's proposition II:	4 votes for; 550 votes against.
Poche's proposition III:	4 votes for; 551 votes against.

A detailed report will be made to the Tenth International Zoological Congress (Budapest) and the undersigned unreservedly accepts the unambiguous results of this referendum as definite instructions from the profession in the United States for him to cast his vote (in the congress as delegate, and in the commission as member) against all three propositions.

C. W. Stiles,
Professor of Zoology, U. S. Public Health Service

"OPALINA ELONGATA" GOURV. IS CEPEDEA SAHARANA METCALF

V. Gourvitsch describes as new an Opalinid from "*Rana ridibunda*" from Tashkent, Turkestan.[1] He names this "*Opalina elongata.*" It is a *Cepedea* and from his description seems to be the form I have described as *Cepeden saharana* from *Rana esculenta ridibunda* collected at Biskra, Algeria.[2] It seems well to call attention to this to prevent confusion.

Maynard M. Metcalf

QUOTATIONS

PUBLICITY AND SCIENCE

In this day of personal horn-blowing it is refreshing to come upon a group of men who are doing great things, yet who shun publicity as they would the plague. As a matter of fact, they would not shun the

[1] V. Gourvitsch: The protozoan fauna of the intestines of frogs from the vicinity of Tashkent—in the *Bulletin* of the Government University of Central Asia, No. 14, 1926. [Russian.]

[2] M. M. Metcalf: "The Opalinid Ciliate Infusorians," United States National Museum, No. 120, 1923.

plague. They are engaged in a fight against cancer, which is far more persistent and no less deadly.

There was an account in Thursday's *Evening Post* of the work accomplished by physicians of the Memorial Hospital, to which Edward S. Harkness recently gave $250,000 for the purchase of four more grams of radium. From this publicity Dr. James Ewing, director of cancer research at the hospital, shied as though one were asking him to give a trapeze performance. About a year and a half ago, when the new treatment was first announced in the press, Dr. Herbert A. Quick, who has directed its application and has had much to do with its development, is said to have acted as though he were being disgraced. Mr. M. Failla, the laboratory worker whose suggestion it was that gold instead of glass be used in the "seed" tubes now implanted in cancerous growths—a vital element in the new treatment never before has been publicly mentioned in connection with the discovery. Nor has Dr. Max Cutler, who first worked out the problem on animals before it was applied to human beings.

There is a deep-seated prejudice against publicity, the heritage of many of the ablest men in the medical profession, which plays its part in the suppression of information which should be presented to the public. One of the most important reasons for this feeling lies in the constant flood of "claims" which second-rate scientists make of "discoveries" and "cures." Some of these announcements are made through overenthusiasm and some through a desire to take advantage of the public's ignorance of science.

Hence the practice has grown up among men of science of not making public announcements of their findings until they have been presented before groups of leading men in the profession, who can discuss and criticize them in the full light of scientific knowledge before the public is informed. This is a wholesome procedure. But the fear of publicity is carried too far when leading men are afraid to speak for publication even after their work has been tested in the scientific society's conferences and in the laboratories of coworkers, simply because they fear criticism from members of their own profession.

It is largely because of the reticence of the men best qualified to speak that those not nearly so well qualified occupy so much of the newspaper space devoted to science. It is also partly because of this reticence that publicists have conceived the notion that the public wants its science information jazzed up and distorted. The public never was so hungry for authentic information as it is to-day. If men of science were to speak freely where they find a disposition to report news of science sanely an *entente cordiale* between science and journalism would be established which would be of inestimable value to both—and to the public.—*New York Evening Post.*

SCIENTIFIC BOOKS

Collected Papers of Sir James Dewar. Edited by LADY DEWAR. Vol. I. 27×17 cm; pp. xxii+674. Vol. II. 27×17 cm; pp. ix+814. Cambridge and New York: The Cambridge University Press; The Macmillan Company, 1927.

To many chemists Dewar is known as the Englishman who specialized in low-temperature work and as the man who invented the thermos bottle. Only relatively few know much about his spectroscopic work in collaboration with Liveing and yet his high-temperature work began in 1872 and was continued up to 1889. Since the first low-temperature paper was published in 1884 and since Dewar was appointed Jacksonian professor of natural philosophy in the University of Cambridge in 1875 and Fullerian professor of chemistry in the Royal Institution of Great Britain in 1877, it is evident that he had made a distinguished name for himself before he ever started on low-temperature work.

New to most people will be the work on the physiological action of light, on electrophotometry, on capillarity and on the properties of nickel carbonyl. A good many people know that he collaborated with Moissan in studying the properties of liquid and solid fluorine.

What very few people realized, outside of his personal friends, is the surprising versatility of the man. When he studied low-temperature problems, for instance, he covered the whole range. It was not merely a question of developing improved methods of liquefying gases and of determining melting points, boiling-points, and densities. Dewar studied specific heats, latent heats of vaporization, diffusion, adsorption by charcoal, optical and magnetic properties, color, photochemical reactions, the effect of low temperatures on bacteria and on electrical resistance, etc., etc. It is a real pleasure to note how many sides Dewar saw to a problem. Everybody knows that Dewar was a marvelous manipulator; and this fact is impressed on the reader because, in these volumes, one runs the whole gamut in so short a time.

It is interesting to note Dewar's attitude towards physical chemistry as far back as 1888. "At the present time we may say that there are two large schools of chemistry: one school cultivating organic chemistry, in which structural or atomic building up of an atomic character is carried out on a gigantic or manufacturing scale, producing thousands of new bodies every year, and continually increasing in the energy of its work and the variety of its produc-